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THE EFFECTS OF EARLY DIFFERENTIAL ENVIRONMENTS ON CERTAIN BEHAVIOR PATTERNS IN THE ALBINO RAT

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The main problem confronting investigators of environment is quantifying the effects of differential early experiences on specific later behavior patterns. Although many investigators (5, 7, 8, 15) agree that early experience has a profound effect on later behavior, there is noticeable disagreement throughout the literature as to which behavioral characteristics are "environmentally sensitive." Beach and Jaynes (1), in a review of the literature, credit the confusion to inadequate experimental design and the small number of subjects employed.

The present study is designed to test the effects of early experience on four behavioral characteristics: emotionality, exploratory behavior, "intelligence," and discrimination learning. Specifically, what are the resultant effects of restricted vs. unrestricted environments on the above characteristics?

METHOD

Subjects

The Ss for this study were members of six litters of Wistar strain albino rats from the Florida State University colony. The mothers were selected at random from the colony, bred, and randomly assigned to one of the two environments. All litter sizes were reduced to eight at birth. At the beginning of the experiment there were 23 animals in the experimental group and 17 in the control group. Subsequent mortality reduced the experimental group to 21.

Environments

The control group was delivered and lived in regular laboratory cages measuring 10 in. x 6 in. x 7 in. There were no more than four animals in any cage, and males and females were separated.

The experimental environment was a special room, 120 in. x 70 in., bounded by a wall 36 in. in height, with concrete flooring covered by one inch of fine sawdust. After puberty the environmental area was divided in half by a 40-in. high wall, with males on one side and females on the other. The regularity of the walls was broken at random intervals by baffles, 6 to 8 in. long, projecting toward the center. Food and water containers were placed at intervals along the walls. The environment also contained 2 T-mazes, 2 Y-mazes, 3 covered blinds, 2 ladders, 2 runways, and 2 bridges, which occupied approximately 57 square inches of floor space each. These were designed to afford a wealth of perceptual, kinesthetic, manipulative and tactual experience. Into this environment the experimental group was delivered and remained there throughout the entire experiment. Mothers remained with their litters, in both groups, until weaning at 21 days of age.

Procedure

All animals received the same test sequence at the same chronological age. At the age of 30 days (prepubescence) the animals were tested for emotionality and exploratory behavior on the Hall *Open Field Test* (6). The measure of emotionality was the number of fecal deposits in each four minute period. The measure of exploratory behavior was the number of grid crossings during the same period (14). At the age of 33 days the animals were placed on 23-hr. water deprivation in preparation for the Hebb-Williams index of animal "intelligence" (9). The animals were permitted to explore the test box, without baffles, for one hour on the next three days; at this time they received water in the goal box. They were then given four trials on each of the next two days, a different problem being presented each day. The last three trials for each problem were scored 1, 2, or 3 depending on the directness of the route taken to the goal, 3 being the score for the most direct route.

At the age of 40 days, all rats were given a light-dark discrimination problem on a single unit water T-maze (see Fig. 1). Prior to running, the animals were habituated to water in a straight water tank. They were then required to swim to the lighted side of the maze. The criterion of learning was 9 out of 10 correct trials. An error was recorded when the animal passed one head-length beyond the choice point on the dark side. Each animal was run for 10 trials a day until the criterion was reached. The stimulus marking the escape or reward arm varied from side to side at random. Each trial was run remotely and the room was darkened; the only light was provided by a 15 watt stimulus bulb located above the choice point.

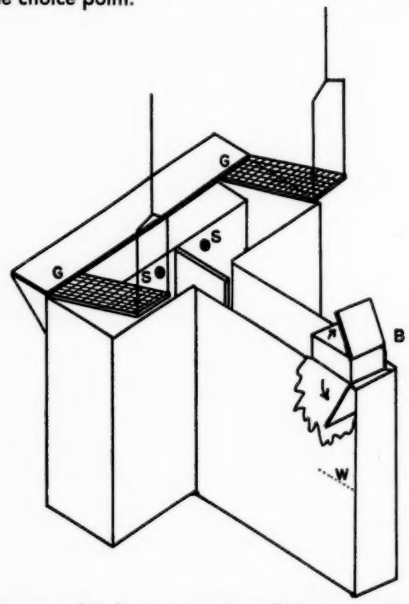

Fig. 1. Water T-maze used in learning tests (overall dimensions: 36 in. x 48 in. x 36 in.). G, retractible escape ladder to goal; S, 15 watt stimulus lamp; B, start box with hinged top and bottom; W, water line.

At the age of 50 days (postpubescence) the animals again were run on another Hall *Open Field Test*. A day later they were brought back to criterion level on the water T-maze. At this time there was no limit to the number of trials they ran in one day. On the following day a negative transfer test was begun. The same water T-maze was used under the same conditions except that the animals were now required to run to the darkened side to escape the water. The criterion for learning was, as before, 9 out of 10 correct trials.

Parameters such as heat and light were held as constant as possible for both groups. However, the experimental animals, in the free environment, were able to make more of an adjustment to their surroundings and thus regulate, at least partially, the amount of heat and light immediately around them.

RESULTS AND DISCUSSION

The test results have been analyzed by the non-parametric, ranking, *H* test (10). Pertinent estimates of central tendency, sample sizes, *H* scores, and probabilities are summarized in Table 1. Each probability was determined by assuming *H* to be distributed as chi square.

From Table 1 it can be seen that there was no significant difference between prepubescent groups when tested for emotionality by the Hall test. This finding conflicts with those of Clarke *et al* (3) and Forgus (5). Although it is recognized that these authors' results were based on different subject types in the case of the former and different procedure in the case of the latter, there seems no reason to doubt that these experiences are all related to environment and that results obtained from one species should not be generalizable to another.

The probability that the present results might be a function of age was considered, and thus the second Hall *Open Field Test* was run. Again, no significant difference was found between groups. Paradoxically, at this testing, the experimental group appeared the more emotional. The experimentals exhibited a stable level of emotional response throughout both tests while the controls exhibited a marked decrease in response to the situation. Table 2 compares the control performance on activity and emotionality measures in the early and later re-test. The lower level of control emotionality was noticed by the *Es* during the test situations. The controls showed less squirming and clawing behavior when handled. That restricted animals should be less emotional than non-restricted animals might be attributed to an "intelligence" factor. For example, Hebb (7) has found that as intelligence level increases the animal will be more susceptible to "emotional weakness." As indicated in Table 1, the experimental group was far more intelligent than the control group as measured by the Hebb-Williams index (9).

The results of the exploratory behavior test (number of lines crossed) show that the experimental group exhibited greater exploratory behavior before puberty, as has been previously demonstrated (3). Postpubescent testing shows the experimental group to be somewhat less active than on the previous test. These results are expected in light of the findings of Montgomery (12) and Berlyne (2), who found that animals spend less time exploring familiar stimuli. However, the control group showed a significant increase in exploratory behavior (Table 2) to the point where there was no difference between the two groups.

TABLE 1

SUMMARY OF EXPERIMENTAL-CONTROL COMPARISON FOR ALL MEASURES OF PERFORMANCE

MEASURE	GROUP	Sum r	Mean	*N*	*SD*	*H*	*P*
Hebb-Williams Index of	E	603	2.2	22	0.44	23.1	< .01*
Intelligence	C	177	1.6	17	0.07		
Hall I	E	613	132.4	22	48.80	24.0	< .01*
Lines Crossed	C	167	32.8	17	26.60		
Hall II	E	447.5	95.7	21	27.80	1.20	< .30
Lines Crossed	C	293.5	81.2	17	34.50		
Hall I	E	423	3.9	22	2.15	1.80	< .20
Fecal Deposits	C	397	5.0	17	3.08		
Hall II	E	466	3.5	21	2.60	2.75	< .10
Fecal Deposits	C	275	2.4	17	2.00		
Original Learning	E	343	49.5	21	30.50	3.81	< .07
(Trials to Criterion)	C	398	77.2	17	39.60		
Relearning	E	543	29.3	21	21.80	8.51	< .01*
(Trials to Criterion)	C	237	10.6	17	14.30		
Negative Transfer	E	395.5	76.5	21	41.0	1.59	< .12
(Trials to Criterion)	C	384.5	103.1	17	53.0		

* Denotes a probability level low enough to warrant rejection of the null hypothesis.

TABLE 2

CONTROL-CONTROL GROUP COMPARISON OF INITIAL AND RETEST PERFORMANCE*

MEASURE	PERFORMANCE	MEAN	*N*	*SD*	CHI SQUARE	*P*
Hall	I	32.8	17	26.6	8.46	.01
Lines Crossed	II	81.2	17	34.5		
Hall	I	5.0	17	3.08	4.40	.05
Fecal Deposits	II	2.4	17	2.00		

* Analysis is by the *McNemar Test for Significance of Changes.*

The lack of postpubescent difference in exploratory behavior between the two groups is consistent with the findings of Montgomery (13), but the reason for the controls' increase is in doubt.

On both original learning and negative transfer tests the experimental group was not significantly better than the controls. This is inconsistent with earlier findings (1, 4, 7), in which it is reported that animals living in a nonrestricted, perceptually rich environment are superior in problem solving to animals living in a restricted environment. It may be argued that, since the probability level for the tests of original learning is nearly low enough to allow rejection of the null hypothesis, there is a definite trend in favor of the superiority of the experimental group.

The relearning test, where the controls were significantly superior to the experimentals, is an excellent example of retroactive inhibition (11). It has been shown repeatedly that increased activity during an interpolated period between original learning and relearning will inhibit relearning. It is reasonable to assume that by virtue of the environmental size alone, the experimental animals participated in a higher level of interpolated activity.

An overall look at the results points up one striking feature. The differences between the groups seem to diminish as the animals grow older. This is contrary to the findings of those who have reported that early experience has a lasting effect on later behavior (7, 8, 15).

If we may assume the relation between performance and experience to be an asymptotic function, then it follows that the experimental animals would be closer to the asymptote prior to testing. Therefore, experience provided by the test situations would affect the performance of the experimental animals much less than the performance of the controls, which were relatively experientially deprived. Thus, increased testing would tend to fill the experiential deficit of the controls, and eventually their performance might approach that of the experimental group.

SUMMARY

Two groups of albino rats were delivered by normal birth and matured in differential environments. One group lived in regular, bare laboratory cages while the other group lived in a relatively unrestricted environment which contained T-mazes, Y-mazes, covered blinds, ladders, runways, and bridges.

At various intervals in their lives the *S*s were tested for emotionality, exploratory behavior, "intelligence," and discrimination learning. The results were as follows:

1. The experimental, unrestricted group exhibited no statistically significant superiority over the control group in the learning of a light-dark discrimination problem. The experimental group showed as much evidence of negative transfer when the stimuli of this problem were reversed.
2. The control group relearned the light-dark discrimination problem, following an interpolated period, more rapidly.
3. The experimental group displayed significantly higher "intelligence" as measured by the Hebb-Williams index of animal intelligence.

4. There was no significant difference between groups with reference to indicated emotionality.

5. The prepubescent experimentals displayed a significantly higher level of exploratory behavior than did their control counterparts; however, at the postpubescent testing no between groups difference in this mode of behavior was found.

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THE LEARNING OF ALBINO RATS AS AFFECTED BY ALTERING THEIR PRENATAL METABOLIC ENVIRONMENT

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Very few studies have been found which deal with the effects of maternal *hyperthyroidism* during gestation upon the learning ability of the first generation progeny. In the earliest of these, Rickey (9) reports that the offspring of female rats which had been receiving 700 mg. of desiccated thyroid gland daily appeared to be normal "in every respect" at birth. When the animals were 25 days of age they were tested on a Watson Circular Maze. Using the time required to traverse the maze as an index of learning, and administering one trial per day, Rickey found that as the number of trials increased, the time required by the experimental animals also increased. In contrast, the control group required less time. She described the behavior of the experimental animals as being "very erratic and nervous" during the maze trials.

Morrison and Cunningham (6) report that they found impaired learning of a conditioned avoidance response in the offspring resulting from maternal hyperthyroidism during gestation. They fed 10 mg. of desiccated thyroid gland, daily, to rats during pregnancy. Five of the first generation progeny acquired a conditioned tail movement to the criterion of 8 conditioned responses out of 10 trials on each of two consecutive days, in about the same number of trials as the control animals. Four of the experimental animals failed to reach the criterion after receiving 80 per cent more trials than the other five experimental animals. On the basis of this, the authors concluded that there was an observable impairment of learning ability in the experimental animals. No explanation was offered, however, for the significant fact that over half of their experimental animals acquired the conditioned response as readily as the normal animals.

Contrary findings are reported by Dispensa and Hornbeck (2). They found that daily injection of pregnant rats with 1/25 grain (approximately 2.4 mg.) of desiccated thyroid per kilogram of body weight improved the maze learning ability of the first generation offspring when tested at 35 to 45 days of age. While the experimental animals were not superior to the normal animals on the first few trials, there was consistently greater improvement in the performance of the experimental animals, so that by the 17th to 21st trials the experimental animals made significantly fewer errors in traversing the maze than the control animals.

No general statement, consistent with the available evidence, may be made concerning the effects of maternal hyperthyroidism upon the learning ability of the offspring. Morrison and Cunningham have attempted to explain the impaired acquisition of the conditioned tail withdrawal by assuming that the lower body weight of the experimental animals indicated that they were cretinous. Kenshalo (3), however, was unable to find differences between the body weight or basal metabolism of offspring of normal and hyperthyroid females. Rickey (9) also failed to note differences in body weight of her animals. On the other hand, Dispensa and Hornbeck found that their experimental animals tended to be heavier than the control animals.

Only one experiment has reported on the influences of maternal *hypothyroidism* upon learning ability of the progeny. Rickey (9) found no apparent learning in the experimental offspring after 10 trials in the Watson Circular Maze. She described their behavior as being "limited," and their movements "slow" and of "low vitality." She also reported that the offspring of the hypothyroid rats were ⅓ to ½ smaller at birth than the normal animals. Kenshalo (3) was unable to confirm this, finding no difference in weight during the growth period.

Further experimentation is indicated to clarify and amplify the relations between maternal hypo- and hyperthyroidism and the behavior of the offspring.

METHODS AND CONDITIONS

Subjects

A detailed description of the Ss and the procedure involved in obtaining the offspring has been given elsewhere (3). Briefly, the *P* (parent) generation consisted of 79 female and 10 male albino rats between 90 and 120 days of age. The Hypo *P* group consisted of 30 females, of which 6 were totally thyroidectomized, 12 had approximately ¾ of the thyroid gland removed, and 12 had only one lobe removed. The Hyper *P* group contained 22 females. During pregnancy six received daily injections of 0.012 mg. of thyroxine; six, 0.025 mg.; five, 0.050 mg.; and five, 0.075 mg. The smallest dose is about 2.1 times the amount normally secreted by pregnant rats of this weight, while the largest is about 13.4 times that amount (5). The Control *P* group consisted of 27 females.

Basal metabolic rates, taken when these animals were not pregnant but subjected to the experimental conditions, were determined by the method described by Morrison (7). The caloric consumption of the Control *P* females was 6.25 Calories per kilogram of body weight per hour (CKH), while that of the Hypo *P* females was 4.97. The *t*-test gave a ratio of 9.48, which was significant well beyond the 1 per cent level of confidence. The basal metabolism of the Hyper *P* group was 7.26 CKH. The difference between the Hyper *P* and Control *P* females was also significant beyond the 1 per cent level of confidence, the *t*-ratio being 6.01. Complete data on these animals has been published elsewhere (3).

As a result of breeding, the Hyper *P* females produced 17 males and 13 females (Hyper F_1 group), the Hypo *P* females, 11 males and 12 females (Hypo F_1 group), and the Control *P* females, 18 males and 19 females (Control F_1 group).

During the period from weaning, at 4 weeks, until 103 days of age when the first test started, the members of the F_1 generation received daily handling. When the test series commenced the animals were quite docile.

Apparatus and Procedure

Motivation. The experimental variables affect metabolism of the *P* generation and possibly also that of the F_1 generation. For this reason tests of the hunger drive and the escape-from-water drive were conducted. Aside from an interest in motivation *per se*, these would be of use in interpreting the results of maze learning should differences occur. The tests of motivation and learning were so arranged that the tests requiring a constant level of hunger drive occurred in succession (Columbia Obstruction Apparatus, Skinner Box, and the Automatic Maze). The time of testing was arranged so that regardless of the test, it fell at the same time of day. The animals were deprived of food for 23 hours prior to each daily trial.

A simplified form of the Columbia Obstruction Apparatus was used as one of the two measures of hunger drive. Five days after starting the feeding schedule the animals were habituated for three days in the apparatus. This consisted of permitting them to explore and obtain their daily food ration in the incentive chamber. The standard diet of Purina Laboratory Chow was presented in powdered form. On the fourth day the test of motivation was carried out. The procedure was essentially that reported by Warden (13). Two deviations from this procedure occurred. First, only the number of grid crossings from the start to the incentive chamber during the 20-minute test period was counted. Second, no shock was employed in the alley. The latter change was based on a study by Margolin and Bunch (4), which indicates that a charged grid is not necessary to provide a valid measure of the hunger drive by this method.

The rate of bar pressing in a Skinner Box was used as a second measure of the hunger drive. The training was essentially as described by Skinner (10). After preliminary training, if the animal failed to accumulate 10 reinforcements during the first 30 minutes it was dropped from the test. A reinforcement was counted when the animal procured the pellet from the food cup within one second after pressing the bar. Ninety reinforcements of pressing the bar were given to each rat over a period of two days. On the third day the animal was placed in the box and was allowed to press the bar for 15 minutes. The number of times it pressed the bar during this period was taken as an index of its hunger drive. The pellets used were made from powdered Purina Laboratory Chow and weighed, on an average, 50 mg. each. None of the rats received more than five grams of food, the mean being three grams, during the fifteen minutes. The animals were between 130 and 150 days old at the time of this test.

Since one of the learning problems was a maze using escape-from-water as a motive, an index of its strength was obtained by measuring the speed of swimming in a straight tank. The tank used was 8 ft.-long, 8 in.-wide, and the water was 18 in.-deep. Ten trials were given to each animal with one minute rest between trials. Between the fifth and sixth trials a rest period of 15 minutes was allowed. In computing the mean time to swim the length of the tank only the last six trials were used. The animals were between 170 and 186 days old at the start of the test.

Learning. Two mazes, a 17 unit self-recording Automatic Maze similar to that described by Tolman, Tryon and Jeffress (12), and a 14 unit Water Maze described by Bunch, Frerichs and Licklider (1), were used to test the learning ability of the animals of the F_1 generation.

The animals were given training in the Automatic Maze following completion of the tests of hunger drive. During training they received their daily ration in the goal box of the maze. The first six days of training were devoted to habituating the animals to the automatic features of the maze as described by Tolman, Tryon and Jeffress (12). At the end of the habituation process the animals were given 18 trials, one per day, on the main maze. The animals were between 150 and 166 days old at the start of the test.

Outside of the experience gained on the speed of swimming test, the animals were given no preliminary training on the Water Maze. Practice was distributed, three trials on the first day and five trials per day for the next three days. The water in the maze was at room temperature, which varied between 75° and 80° F. It was 18 inches deep, forcing the animals to swim from beginning to end.

In both mazes time and error scores were obtained. In the case of the Automatic Maze, time was recorded from the instant the animal entered the stem of the first T-unit until he entered the true path of the seventeenth T-unit. In the Water Maze time was recorded from the moment the animal was put in the water until its foreleg touched the escape ladder at the end of the maze. The errors in the Automatic Maze were defined by the closing of electrical contacts caused by movement of the rat onto the movable floor in the blind and the stem of the T. In the Water Maze an error was defined as reversing direction and swimming at least ¾ of a body length in the true path or by entry of ¾ of the body into the blind.

RESULTS

Analysis of the data on all tests of differences between the treatment levels within the experimental groups showed none to be significant. The data were, therefore, grouped according to the major treatments.

Motivation

Table 1 shows the means and standard deviations obtained on the three tests of motivation.

TABLE 1

MEANS AND STANDARD DEVIATIONS FOR THE TESTS OF MOTIVATION

GROUP	COLUMBIA OBSTRUCTION APPARATUS Grid Crossings in 20 Min				SKINNER BOX Bar Presses in 15 Min.			WATER TANK Swimming Speed (Sec.)			
	N	Mean	SD	t	N	Mean	SD	N	Mean	SD	t
Hyper F_1	30	55.52	26.29		19	58.56	11.28	30	5.91	0.93	
				1.07							3.61**
Control F_1	37	65.05	34.25		34	57.62	20.90	33	6.74	0.87	
				2.51*							3.77**
Hypo F_1	23	43.17	27.01		25	60.67	16.99	21	5.65	1.09	

*Significant beyond the 5% level of confidence.

**Significant beyond the 1% level of confidence.

Analysis of variance of the number of grid crossings made during 20 minutes in the Columbia Obstruction Apparatus gave an *F* of 3.89, which is significant at the 5 per cent level of confidence. As shown by the *t* test in Table 1, the Hypo F_1 group made significantly fewer crossings than the Control F_1 animals. The Hyper F_1 and Control F_1 animals did not differ significantly.

The means of the rate of bar pressing for 15 minutes did not differ significantly as shown by the analysis of variance.[1]

Comparison of the mean swimming speed of the three groups in the straight tank by analysis of variance gave an *F* ratio of 6.68, which is significant beyond the 1 per cent level of confidence. Table 1 shows that both experimental groups were very significantly faster swimmers than the control group.

[1]The assumption of homogeneity of variance is not satisfied in this analysis. However, since the *F*-ratio is not significant, even with the heterogeneous variance, and since the differences between the means are relatively small, a transformation of the data does not appear to be necessary. A Kruskal-Wallis one-way analysis of variance by ranks test does not yield a significant *H*.

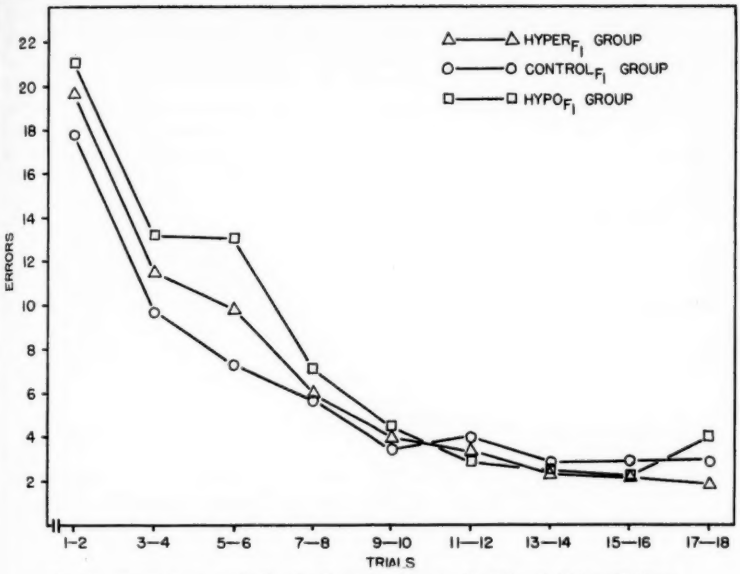

Fig. 1. Mean errors made by the F_1 generation on the 17 unit Automatic Maze.

Figure 1 shows the progress of learning of the F_1 animals in the Automatic Maze during 18 trials. Both experimental groups tended to be slightly slower in learning the maze during the first 10 trials than the control group. The differences between the groups are not large enough, however, to be significant. This is supported by the analysis of variance of the total error and time scores. The *F* ratios of 1.11 for errors and 1.72 for time[2] are both not significant.

TABLE 2

MEAN ERROR AND TIME SCORES ON THE AUTOMATIC AND WATER MAZE FOR 18 TRIALS

GROUP	AUTOMATIC MAZE					WATER MAZE					
	Total Errors			Total Time (Min.)		Total Errors				Total Time (Min.)	
	N	Mean	*SD*	Mean	*SD*	*N*	Mean	*SD*	*t*	Mean	*SD*
Hyper F_1	26	126.0	46.31	32.28	16.41	30	128.8	60.60		28.67	15.47
									0.11		
Control F_1	30	120.4	41.05	31.05	10.80	32	130.8	75.51		28.66	17.26
									2.01*		
Hypo F_1	21	136.3	52.53	39.44	18.59	22	173.6	75.22		35.35	19.09

*Significant at the 5% level of confidence.

[2]In this analysis also, the assumption of homogeneity of variance is not satisfied. Again, however, since the *F*-ratio is not significant, even with the heterogeneous variance, a transformation of the data does not appear to be necessary. A Kruskal-Wallis one-way analysis of variance by ranks test does not yield a significant *H*.

From an examination of Fig. 2, showing the performance of the groups on the Water Maze, it is evident that a large difference occurred in the performance of the Hypo F_1 and Control F_1 groups. The Hypo F_1 group apparently did not profit much from the 18 trials in the maze.

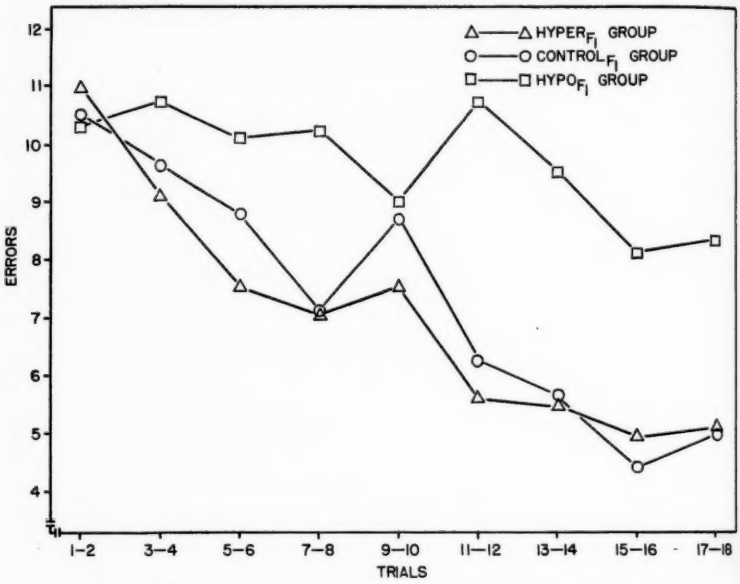

Fig. 2. Mean errors made by the F_1 generation on the 14 unit Water Maze.

Analysis of the variance of the Water Maze results showed that a significant difference existed between the groups for errors but not for time. The *F* ratio for time is 1.24. For errors it is 10.22, significant beyond the 1 per cent level of confidence. As shown in Table 2, comparison of the means by *t* test indicated that the Hypo F_1 group made significantly more errors than the Control F_1 animals. No statistically significant difference was noted between the Hyper F_1 and Control F_1 groups.

DISCUSSION

The lack of a significant difference in the performance of the Hyper F_1 and Control F_1 groups on the various tests used in this study is not in agreement with the studies reported earlier. It will be recalled that both Rickey (9) and Morrison and Cunningham (6) report impaired learning of a maze and a conditioned avoidance response, respectively, in offspring of hyperthyroid female rats. They have both attempted to explain their results by assuming that the young were cretinous. It seems doubtful, however, that maternal hyperthyroidism produces cretinism in the offspring. Certainly, the Hyper F_1 animals of this study were not cretinous as shown by growth rate and basal metabolism (3).

One possible explanation for the lack of agreement between this and Rickey's report is in terms of the size of the dose of thyroid substance administered. A rough comparison between the dosages may be made on the basis of the iodine content of the thyroid material. The *U. S. Pharmacopoeia* (11) states that dried thyroid shall have between 0.17% and 0.23% iodine as an integral part of the thyroid substance, whereas thyroxine shall have not less than 64%. Rickey, in administering 700 mg. of desiccated thyroid daily, was administering the approximate equivalent of 2.17 mg. of crystalline thyroxine. According to Monroe and Turner (5) this is about 181 times the normal daily production of pregnant albino rats. It seems probable that hyperthyroidism was complicated by a toxic condition.

By the same comparison Morrison and Cunningham's daily dosage falls within the limits of this study. They administered 10 mg. of desiccated thyroid per day, which would be roughly equivalent to 0.031 mg. of crystalline thyroxine daily or approximately 2.6 times the normal daily production. The question remains, however, whether slower conditioning in four of the nine experimental animals used in their study demonstrates impairment due to having hyperthyroid mothers.

Examination of Fig. 1 shows that from the 11th through the last trial the Hyper F_1 animals tended to be superior to the Control F_1 animals. It is conceivable that had the trials continued they would have shown significantly superior performance. In this case the performance of these animals would be in agreement with that reported by Dispensa and Hornbeck (2).

The fact that the performance of the Hypo F_1 group did not differ significantly from that of the Control F_1 animals on the Automatic Maze, but was markedly inferior on the Water Maze, suggests that some factor other than learning *per se* must account for this difference. It seems unlikely that this factor could be motivation to escape-from-water, since *both* Hypo F_1 and Hyper F_1 animals swam the length of the water tank significantly *faster* than the Control F_1 group, but only the Hypo F_1 group was *poorer* in learning.

The difference observed in performances on the Water Maze but not on the Automatic Maze may be explained if one assumes that being dropped suddenly into the water produced more of a disrupting effect on the maze solving behavior of the Hypo F_1 group than on the Control F_1 and Hyper F_1 groups. This might be thought of as a difference in emotional responsiveness.[3]

The slightly inferior performance of the Hypo F_1 animals on the early trials of the Automatic Maze and subsequent equal performance in later trials is suggestive of adaptation to the situation, a process which might be expected to occur if the Hypo F_1 group was more emotionally responsive. The magnitude of the effect was probably reduced considerably by the preliminary training which was given before the actual test in the Automatic Maze. The faster swimming speed of the Hypo F_1 group and the notion that swimming is an emotion provoking situation is in line with Parker's work (8) with water immersion as a test of emotionality.

SUMMARY

The performance on tests of motivation and learning of offspring of female hypothyroid and hyperthyroid albino rats was compared to that of normal offspring. The three tests of motivation were the number of crossings in a Columbia

[3]The term is intended to imply nothing more than the physiological changes which take place in an organism when confronted with an unusual or novel situation.

Obstruction Apparatus, the rate of bar pressing in a Skinner Box, and the speed of swimming a straight water tank. The tests of learning were a 17 unit Automatic Maze of Tolman, Tryon and Jeffress design, using hunger as a motive, and a 14 unit Water Maze of Stone's design using escape-from-water as a motive.

It was found that the Hypo F_1 group made significantly fewer (5 per cent level of confidence) grid crossings in the Columbia Obstruction Apparatus than the Control F_1 group. No significant differences were found, however, in the rate of bar pressing in the Skinner Box. Both experimental groups swam the straight tank faster than the Control F_1 animals. The three groups did not differ significantly in performance on the 17 unit Automatic Maze. On the Water Maze the Hypo F_1 group made significantly more errors during 18 trials than did the Control F_1 animals. No statistically significant difference was found for time. The Hyper F_1 group did not differ significantly from the Control F_1 group.

In order to explain the differences in performance of the Hypo F_1 group on the Water Maze and the lack of any difference on the Automatic Maze it is suggested that the Hypo F_1 animals were more emotionally disturbed by the Water Maze than the Control F_1 and Hyper F_1 animals.

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PERSPECTIVES IN PSYCHOLOGY

VIII. THE THREE WORLDS OF *DASEINSANALYZE*

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During its none too lengthy past psychology has probably been challenged as often as any science having a more ancient lineage, a circumstance which leads one confidently to expect many more such challenges in the future. A wise science, like a wise parent, will utilize such dissents as an opportunity to re-examine its assumptions and methods with a view towards bettering itself. Such an opportunity exists at the present moment with the advent on the European stage of the twin philosophies of phenomenology and existentialism. These two philosophies, the former springing from Edmund Husserl, and the latter primarily from Soren Kierkegaard and Martin Heidegger, among others, are so intertwined with one another in the writings of their contemporary practitioners that it is difficult to disentangle them. Various as the individual contributions are, there are certain attitudes towards psychological science held in common (4, 5).

Due to the fact that psychology has emulated the natural sciences, it has come to look upon man as an *object* analogous to the objects that natural science studies, and has applied to man the same methods of analysis, categorization, and measurement. But the human *subject* cannot be understood in such terms; to do so is to reduce him to something he is not, and thereby to miss the fundamental difference between man and the world. The existence of every man is deposited in a phenomenal world of his own, and it is this world which must serve as the starting point for our comprehension of what man is like. We do not need to analyze the phenomenal world of a stone in order to understand it — indeed we do not even presume that it has one — nor is such an analysis called for to any extent in the biological sciences. But for man himself the case is different. His existence in a phenomenal world of his own is the sum and substance of his psychology, and thus forms the indispensable basis for comprehending any aspect of his life. This statement is true in a double sense — from the standpoint of the person being studied, and equally from the standpoint of the human being who is doing the studying.

According to the existentialists, if we approach such a phenomenological analysis freed from the damaging assumptions inherited from the physical sciences, we will see that man has no fixed qualities or *essences* such as a stone has. Rather is he to be understood simply as a concrete, undetermined existence moving towards a future (in the sense of a project) (3). Consequently, man is characterized above all as possessing transcendence, including self-transcendence, which is to say, in part, that he is capable of surpassing or standing beyond any sort of presumed "cause" which is assigned to his psychological makeup. Man is not the result of his biological makeup, or of his culture, or of his history, or of any combination of these things; at least he is not the result of these things unless he falls into the inauthentic mode of existence of treating himself as if he were an object. His existence is based upon freedom to choose,

but since choice is made in the face of nothing that can serve as an absolute guide, man is continually faced with anxiety from which he tries to escape.

A concrete example of the phenomenological-existential approach to the study of man is contained in the theories of *Daseinsanalyze*, a form of existential psychotherapy developed by Binswanger, Boss, and their students. When they are actually engaged in the analysis of case histories of abnormality (2), the practitioners of this branch of psychotherapy are apt to talk of the manner in which the patient has related himself to his own *Umwelt*, the natural and biological world; to his *Mitwelt*, the world of other humans like himself; and to his *Eigenwelt*, his own self-world. The meaning of this threefold distinction, commonplace enough in itself, must be understood against the background of the ideas summarized above.

To the writer's way of thinking, the essential message of *Daseinsanalyze* is that we must learn to understand how the patient has chosen to actively relate himself to these three worlds, not what these worlds, *as facts*, have caused him to become. We are faced with a world-picture, so to speak, actively held by the patient, which the therapist and the patient himself must come to appreciate. If the encounter with the therapist has been a vital one (one in which the phenomenal worlds of each are changed), the patient may again be able to face his existence with the freedom to choose, that is, the freedom to transcend himself, to become other than he is. Neurosis is one of the several ways by which man, as a result of anxiety, loses command of his self-transcendence. *Daseinsanalyze* is critical of standard Freudian psychoanalysis and of most of the neo-Freudian derivatives. Standard psychoanalysis was mainly a biological determinism modified by the vicissitudes of life events, while its newer forms have introduced more of a cultural determinism. But all scientific determinisms, because they miss the insight into man's transcendence, tend to reduce him to a passive, externally caused, object and rob him of his freedom to become creatively new and different. Man surpasses both his biological status in the natural world and his history.

What kind of a perspective can we gain on the philosophy of *Daseinsanalyze*? If we begin by searching for what school of thought in contemporary psychology it most resembles, the various field-theoretical systems immediately come to mind, especially the transactional view of Dewey, the interbehavioral psychology of Kantor, and the psychological system of Lewin as developed by his students. In the system of Kantor, for example, stimuli and responses are simultaneously operating things comprising the two parts of unitary events, so that one cannot rightfully say that either part "causes" the other to occur. The interbehaviorist is thoroughly accustomed to speaking of the psychological organism as actively endowing stimuli with special functions (i.e., with meanings). There is no clearer example of this than the activity of a subject in endowing the ink blots of the Rorschach test with definite stimulus functions — these functions being as much a product of his own psychological constitution as they are of the physical stimuli themselves. Existentialism tends to emphasize almost to the exclusion of everything else this active constitution of his environments in which the psychological organism engages. The "phenomenal worlds" of phenomenology-existentialism are the stimulus functions of objects that have come into existence for an individual subject as a result of his life activity. But the psychologist looks upon these phenomenal environments of humans as part and parcel of the natural world, not as its metaphysical substrate.

When we approach the question of causation in the temporal sense of the word, that is, the relation of the individual's past to what he is at the present time, another parallel to the transcendence doctrine of existentialism can be found. Modern psychology is by no means as thoroughly wedded to the classical metaphysical view of monistic materialism as its critics accuse it of being. Large numbers of contemporary theorists in psychology look upon their views as non-reductionistic, meaning by this that they do not think psychological facts and principles can be reduced to the level of neurological description, or beyond this, to the laws of chemistry and physics. In fact, the entire structure of modern science is far removed from the "reductionistic ladder" of classical materialistic metaphysics; it resembles more a series of organizational levels, each with its own realm of data and principles ordering these data, wherein the facts of a more basic science become incorporated into a different pattern of events. Using such a system of thought, there is no compulsion for psychologists to biologize human nature; they may be more inclined to "psychologize" biological processes, which were traditionally assumed to operate in independence of psychological influences.

Within the limits of psychology itself, the same concept may again be applied to distinguish levels of personality which represent different orders of organization. One such system is the ego structure which, once having come into existence, functions as an encompassing unity into which more elementary processes are assimilated. Whenever the person gains from his significant encounters with other persons a new self-attitude, he is in the position of changing the effect which his previous life experiences have had upon him. To a large extent then, the person's attitude towards his past life is as important as that past life was in itself in determining what kind of an individual he now is.

What kind of a philosophy lay behind these developments away from the reductionistic views of classical materialism? The writer believes that it is to be found in the philosophy of emergent evolution, which was so widely influential during the early decades of this century. This philosophy looked upon the universe as a dynamic, evolutionary process in the course of which new orders of existence continually emerged. Life and "mind" were two major stages of emergence which therefore needed distinct sciences to unravel their organizational principles. The emergent evolutionists, however, rapidly got entangled in the meshes of spirit-matter metaphysics, so that their emergents came to look more and more like supernatural entities laid on top of the natural world.

The second great influence that aided in the development away from classical materialism was American naturalism, which insisted, firstly, that *all* the phenomena of man and nature should be studied in detail as fully developing processes and, secondly, that these processes, including mind, were definite spatial-temporal organizations of activity. The world might be heterogeneous and "pluralistic," but it certainly was not composed of a metaphysical matter opposed by a metaphysical spirit.

Such a philosophic framework, applying equally to the physical, biological, psychological, and social sciences, has certain advantages over phenomenological-existential philosophy. The phenomenal worlds of the existentialists skirt dangerously on the borderlands of metaphysical dualism, even though they claim to have destroyed the antinomy of subjective-objective by digging underneath such distinctions. Binswanger, however, still finds it necessary to talk about the "real" environments his patients live in, in contradistinction to their phenomenal

worlds (2, Pp. 237-364), although the relation of the two, causal or otherwise, is never clearly defined. Furthermore, philosophers of existence seem too insistent upon man's uniqueness which requires special metaphysical statement, and too ready to grant to physical science full sovereignty over its kingdom so long as its ultimate base is conceded to rest in the phenomenal world of man. Man becomes constitutive of everything else, and by that very token becomes an insolvable mystery to himself. In this respect, and despite its protestations to the contrary, phenomenology-existentialism gives the distinct impression of perpetuating the traditional dichotomy of man-world, and in so doing misses the chance to become a genuine philosophy of science. Perhaps this explains its attachment to the areas of humanism and religion. On the other hand, the questions it raises concerning the nature of causation, when applied to human psychology, are very important ones which are well worth the attention of psychological science.

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SOME COMMENTS ON EXPERIMENTAL AND OBSERVATIONAL APPROACHES TO BEHAVIOR[1]

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It is perhaps useful at the beginning of such a discussion to admit one's biases and limitations. I am strongly committed to the study of behavior of small, furry, non-articulate, white mammals by the experimental method. Also, I have had very little direct experience with non-experimental techniques and, accordingly, I shall be even more naive when commenting upon these methods.

How can we classify the methods of psychology? While we would all agree as to what we mean by the experimental method, the phrase, "non-experimental approaches," evokes less consensus. This state of affairs is reflected in textbooks of general psychology, where the experimental method is always considered in some detail, but non-experimental methods are either treated in a cursory fashion or are omitted. For purposes of discussion the non-experimental methods may be grouped into the following: (1) naturalistic observation, (2) correlational methods, and (3) clinical methods. Because of time demands, the focus of this discussion will be upon the experimental method and the method of naturalistic observation.

What are the distinguishing features of these methods? What are their limitations and advantages? Let us begin with the experimental method. Skinner has said: "In psychology, as in any science, the heart of the experimental method is the control of the thing studied. When we say 'let us try an experiment' we mean 'let us do something and see what happens' " (6). Skinner has clearly specified two features of the experimental method: first, the degree of control which the experimenter seek to achieve, and secondly, that he *imposes* a variable or variables in order to discern the behavioral consequences. Control of the situation consists of rendering other variables ineffectual as far as is possible by means of their elimination or randomization. An independent variable may be manipulated by varying the stimulus situation or by selecting subjects who differ along a dimension like chronological age, anxiety level, and the like. Parenthetically, it might be noted that Underwood (9) has recently presented an excellent discussion of some of the difficulties which are inherent in the latter approach.

An experiment is characterized by a simplicity on both the independent and dependent variable sides. The experimenter, in the limiting case, may seek to vary only a single independent variable, and rather than record the "total behavior" of the organism, he will often select only a single response measure as his dependent variable. The experimenter will also be likely to choose a dependent variable that provides for a high degree of inter-observer agreement. Of necessity, then, an experiment is *artificial* and *over-simplified*. And furthermore, the artificiality and simplicity of an experiment are arranged

[1]This paper was presented at the Symposium on Experimental Versus Non-Experimental Approaches to the Study of Behavior, held at the annual meeting of the Kansas Psychological Association in Wichita, Kansas, on April 19, 1958.

deliberately by the experimenter (7). The value of this controlled experimental procedure in Cronbach's words is that: "It thus permits rigorous tests of hypotheses and confident statements about causation" (2, p. 672).

The experimentalist and correlational psychologist have been interested traditionally in quite different aspects of behavior. This situation has been aptly described again by Cronbach: "Individual differences have been an annoyance rather than a challenge to the experimenter. His goal is to control behavior, and variation within treatments is proof that he has not succeeded. Individual variation is cast into that outer darkness known as 'error variance' " (2, p. 674). Conversely, "the correlational psychologist is in love with just those variables the experimentalist left home to forget" (2, p. 674).

We have already indicated that the experiment is an ideal medium for testing the deductive consequences of a theory. Experimental paradigms are useful too in another theoretical aspect — the specification of concepts. Underwood (9) has presented a number of examples of how terms like pseudo-conditioning, transfer, and the like may be defined in a relatively unambiguous fashion by reference to differences in performance between experimental and control groups which have undergone different procedures.

A major difficulty with the experimental method concerns the problem of extrapolating relationships found in a controlled situation to the complex conditions of everyday life. It is, of course, the experimenter's hope that principles isolated in the laboratory will be valid outside the laboratory. He cannot solve this problem, however, by wishing it to be so. Nor can the critic of the experimental method decide arbitrarily that laws developed in the laboratory will not "work" outside. Whether or not a law is useful beyond its origin is an *empirical* question. It should be noted that this problem is not unique to psychology, but is present in other disciplines as well. The physicist, for example, continually arranges unreal experimental conditions and, thereby, has achieved a high degree of success (7).

In summary, the experimental method is characterized by a deliberate simplification of variables, by the active imposition of a variable or variables which are of interest to the experimenter, and by an interest in treatment variance rather than variance due to individual differences. It permits the determination of functional relationships, is an ideal technique for theory-testing, and provides for a method of defining concepts. The most serious difficulty concerns the generalization of findings beyond the original controlled experimental conditions. This question was asserted to be an empirical one.

Next let us turn to a brief examination of the method of naturalistic observation, where experiments in nature are described. This method has been championed by Barker and Wright (1) and recently has been discussed by Pauline Sears (5). The first authors contend that while we know a great deal about behavior in experimental and clinical settings, we lack information about the "base rates" for various important psychological phenomena occurring in the home, the school, and so on. They also stress that "it is often impossible to create in the laboratory the frequency, the duration, the scope, the complexity, and the magnitude of some conditions that it is important to investigate" (1, pp. 2-3).

Barker and Wright have set forth with great clarity a number of difficulties in the method of naturalistic observation. The first of these is to obtain observations without influencing the behavior and habitat. Second, a method of

describing the behavior and psychological environment is needed that does justice to the "richness" and provides for the possibilities of quantification. A third methodological problem is to secure "theoretically neutral data" which might have relevance for many problems.

As you know, one of the techniques utilized by Barker and Wright is the day-long behavior record, which was obtained by six to eight trained observers who were rotated every half-hour. This method of data collection results in a mass of information, since a specimen record for a single child may run as much as 300 pages. There are numerous important problems of methodology here. Are the records reliable? Does the observer's presence affect the subject's behavior? Is this sample of the child's behavior representative of his behavior in general? How much interpretation should the observer do? On the problem of reliability it is said: "We are unable at this time to present data bearing upon the reliability of specimen records" (1, p. 213). There are really two reliability questions here. One is the complicated question of the reliability of the specimen record itself; the second, is the reliability of the *coded* specimen record. While evidence is presented for the consistency of the coded data, this does not, it seems to me, solve the first problem. This brings out a difference in the data arising from field studies and that from the experimental method. In the first instance, if total behavior is recorded, it must go through a subsequent coding phase. In this coding phase, itself, there are additional methodological problems, some of which need clarification. The impression should not be conveyed that Barker and Wright are unaware of these difficulties. They have carefully considered all of them, and *Midwest and its Children* is the best available source book on naturalistic observation.

The problem of generalization of results is less complicated in this method than it is in the case of the experimental method, for the behavior occurs in the situation where one wishes to make predictions. If the sampling of subjects is representative, if the specimen records and coded specimen records are reliable, then valid inferences could be drawn regarding the behavior of *Midwest's* children. With replication of these procedures in other communities and cultures, the scope of these inferences may be increased.

Two advantages of naturalistic observation in addition to those given above have been stressed by Hilgard: first, the data which are obtained can lead to some fresh ideas on psychological problems (3), and second, the data can be used to check upon the adequacy of theories developed on the basis of observation in more limited situations (4).

In summary, the method of naturalistic observation results in data which cannot be obtained by other methods and which may be analyzed in numerous ways for different problems. It provides for the possibilities of new insights regarding behavior and serves as a medium for checking limited theories. In the case of specimen records of total behavior there exist methodological problems concerning the reliability of the specimen record, the influence of the observer upon the behavior and habitat, and in coding the specimen record. The problem of generalization was seen to be a less difficult one than is present in using the experimental method.

This symposium has been instigated by Dr. Swartz's provocative paper (8) in which the validity of the experimental method for the study of behavior has been questioned. The question must, however, be raised: valid for what? What method of inquiry is useful is dependent upon the goal of the research worker,

the state of development of the problem-area being investigated, and even such mundane things as the size of the research budget. If the research worker is operating within the confines of a formalized theory in a developed problem-area and wishes to test a deduction from the theory, then the experimental approach is ideal. If, on the other hand, he wishes to explore a new problem-area, he doesn't — to use Feigl's phrase — "need a razor blade to cut butter." If he is *seeking* hypotheses and concepts, he might fruitfully employ less controlled methods of observation with the attendant advantage of securing more information. Finally, while we know something about the assets and liabilities of the experimental method, the full potentialities of naturalistic observation have not been fully explored as yet.

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ON THE RELATIONSHIP BETWEEN EXPERIMENTAL AND NON-EXPERIMENTAL METHODS IN PSYCHOLOGY[1]

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To be a member of the scientific community means to select a certain common value system. It means choosing truth as a value. On this point all psychologists must naturally agree. Consequently, they also have to agree on employing the best possible method for establishing truth, and — ultimately — this must be an experimental method. Differences of opinion regarding the use of experimental and non-experimental approaches may, however, arise on two counts: first, what does it mean to say that ultimately validity has to be based on experiment, and how soon or late is "ultimately?" Second, is there only one definition of experimental method? It is here that psychologists have differed both on the methodological criteria of experimentation and on the *priority* value they assign to it. Underlying these differences are certain value judgments. They range from a preference for studying the experiences and actions of persons to a preference for the exclusive study of behavior "of rats and men." Think, for example, of a clinician and a Skinnerian.

Now, psychology is a very young science. According to Spence, it only began in 1914. Be this as it may, psychology, in the empirical sense, is in a rather early stage of development. Therefore, it seems quite premature to dictate to the psychologist the value judgments he should make in selecting what should come first, experience and action of persons, or, "pure" behavior, couched in the impersonal data language of stimulus and response. At the current stage of psychology it seems premature to legislate which method the individual psychologist should espouse. For example, whether at this point a psychologist adopts the hypothetico-deductive method or whether he proceeds from empirical findings to conceptual generalizations, should be left to his own choice.

We have witnessed, however, in recent years the emergence of a philosophy of science in the form of logical positivism which proclaims the unity of science, including psychology. This unity is to be based upon physics, making physics the model for all scientific enterprise. "Operationism," as the method also proposed for psychology, insists that concepts have validity only if they are anchored in concrete operations which can be experimentally performed, are repeatable and public. "Concepts" have even become synonymous with the corresponding set of operations themselves: experimental procedure and measurement.

This theorem has met with some difficulties in its own field of origin, the philosophy of science, where critics have wondered whether Operationism may involve a hidden metaphysical bias, the very bias of which it wants to rid itself and others. Thus, when the Operationist talks about forces, molecules, atoms, etc., does he really mean that he talks only about scientists and the operations they perform, so that molecules do not really exist but the *measuring apparatus* does really exist? For psychology this would mean, let us say, that the room we

[1]The larger part of this paper was presented at the Symposium on Experimental Versus Non-Experimental Approaches to the Study of Behavior, held at the annual meeting of the Kansas Psychological Association in Wichita, Kansas, on April 19, 1958.

are now sitting in does not really exist, but only the measurements we have taken of it. Of course, the Operationists have introduced remedies to overcome these criticisms. We are all familiar with the introduction of "intervening variables," of "constructs," of "dispositions," any one of which now takes the place of the formerly so-called "real" psychological processes, for instance, "internal" thought and consciousness. This is not the place to deal with the related attempts of formulating theoretical networks of such constructs, "homological" networks which now, according to Feigl and others, need to be anchored only partly in concrete operations. Instead I would like to call attention to some of the consequences of Operationism for current psychology, particularly its influence on the research attitudes of psychologists.[2]

To begin with, Operationists claim a monopoly on the truth value of their method by ruling that all other methods are unscientific, lacking the discipline of physics. Though somewhat shaken, the non-operational, "tender-minded" psychologist has not in turn placed a value judgment of "non-scientific" on the Operationists. Their one-way street of legislating truth has, however, led to a crossing of the roads in answering our question on "is there only one definition of experimental method?" Here the main issue in psychology centers on the scientific status of *direct* experience, of so-called consciousness or, of the phenomenal world of the subject, not to speak of non-conscious events. Operationists object to the experimental use of direct observation of the individual's own experience and to the theoretical use of data of experience, because such data are merely subjective and absolutely "private," hence, non-compatible with the required super-individual, "public" character of science. Here it is not enough to argue that the subject's verbal or written reports have become acceptable to the Operationists, who treat them as responses which are recordable and therefore "protocol" or "public" data. For the contents of the individual's experience to which the words refer, that is, the *experiential referents* of these words, are not accepted as legitimate data, being merely private. Neither is the *content* of a perception admissible evidence, since perception is "regarded as nothing more nor less than a discriminatory response," i.e., the *"reaction is the perception;"* the latter has "no other operationally-determinable properties" (6, p. 149). These formulations do not set up "a straw man" inasmuch as their authors are Operationists. They themselves try to liberate Operationism from such a "narrow" and "sterile" point of view by proposing the use of additional ("converging") operations. The latter should serve to identify those properties of perception that "are distinct from responses," namely, perceptual *contents* (6, p. 158). Such a distinction is still dubious to Boring. It seems paradoxical that in a recent symposium on 20th century psychology (14) Huxley, the biologist, had to argue against Boring, the psychologist, for the right to refer to perceptual contents in the study of animals and men. Huxley proposed that the emergence of new sense organs during evolution produced a vehicle for new contents in the perceptual experience of organisms; and that these new perceptual contents in turn led to the emergence of new behavior.

Boring questioned such a scheme because perceptual experience cannot be dealt with operationally, nor can introspection reliably report it; e.g., two people always call green "green," but one is color blind. This contention invites the following comments: it implies an unproven universal inadequacy of introspection based on untypical instances of error; it really attacks the outmoded intro-

[2]This seems also appropriate in view of Boring's statement. "Operationism is not a school or method. It is a frame of mind" (3, p. 175).

spectionism of Titchener, since contemporary psychologists readily control for such inadequacies by experiment, e.g., with the Ishihara Test; it ignores the basic reliance on perception even in the case of the Ishihara experiment where, e.g., the color blind reports another number in the place of the correct green number, the reported number being again a percept, not a mere "response." (Interestingly, Boring's argument speaks of calling green "green," and of color blindness, thereby referring to perception and misperception.)[3] Finally, such reasoning neglects the fact that the color blind's use of the word "green" and his failure to report the green test-number is in itself a scientific problem. Just as any other discrepancy between reported experience and actual events (or behavior), such fact requires recognition and explanation, but not exclusion.

The entire issue of "subjective" versus "objective" in the private-public controversy concerning direct experience is by no means such a closed issue as the Operationist contends. Ultimately, he himself has to use a phenomenological method to decide by "inter-subjective" comparison whether, let us say, the report of a given pointer reading refers to a public or private perception. If he can agree with others on *his* perception, it will have public status; if he can't, it will have private status. Evidently such agreement or disagreement does not make a perception "more or less" the experience of a *subject*. Thus the terms "public-private" cannot dispose of the basic subjectivity of any experience in the sense of a-subject-having-an-experience; nor can these terms be used to clarify the difference between an experience that refers to an event within the subject and an experience that refers to an event in an object. It is here that the phenomenological analysis of the properties of the phenomenal field becomes decisive. Thus, MacLeod, with other Gestalt psychologists, has pointed to the fact that the phenomenal field as directly experienced has both subjective and objective reference.

> We run the danger of confusion . . . if we speak of the phenomenal world as though it were a "private" world. If we had only private worlds we could have no communication. . . . For each of us there are some phenomena of a private or subjective nature but together we live in a public world (8, p. 236). . . . objectivity and subjectivity are phenomenal facts . . . just as valid as brightness and loudness; they are properties of an organized perceptual field in which the points of reference are selves (subjects) and objects (7, p. 200).

If the directly given already contains discriminable objective and subjective properties and relationships, one cannot call *all* direct experience private. The Operationists, in spite of their rigor, have not yet sufficiently determined the extent to which there is and there is not a lawful communality in our direct experiences of physical objects, internal states, and of interpersonal behavior. They have neglected the prerequisite analysis of the conditions which make for a shared phenomenal, behavioral and social field of organisms as human beings. The foundations for such analysis have been already laid by men like Kohler, Koffka, Asch, Michotte and Heider, to mention only these.

Moreover, even the private data may be not as inaccessible and scientifically unqualified for psychological investigation as has been claimed. To relegate what is private to the realm of the scientifically irrelevant may leave out what is psychologically relevant. Bridgman himself had this to say on the subject:

[3]Boring (4) seems to have accepted introspective reports more leniently for psychophysics.

> Simple observation shows that I act in two modes. In my public mode I have an image of myself in the community of my neighbors, all similar to myself. . . . In the private mode I feel my inviolable isolation from my fellows. . . . The operations by which I now deal with the "public for me" are qualitatively different from the operations by which I now deal with the "private for me". . . . Since one aspect of the structure of my universe is the operational difference between mine and thine, I must make my report in a language which recognizes this operational duality. . . . it seems to me that by ignoring the dichotomy psychology is engaging in an unnecessary gamble and is probably riding for a fall (5, p. 282 ff.).

Bridgman goes on to say that it depends upon the subject matter of a discipline whether or not the private and social modes of individual behavior should be recognized and dealt with. One approach to bring the "private mode" within reach of objective study, without the use of introspection, is the "inferential reconstruction" of the person's phenomenal field or world as proposed by Snygg and Coombs. Sherif's recent efforts have contributed to such reconstruction by manipulating social variables in real life situations. Of course, first and foremost it was Lewin who had paved the way for dealing with psychological and experiential data in their own right by introducing the concept of life space into the orbit of experimental psychology. And, Carl Rogers paralleled and supplemented these accomplishments with his systematic use of the self concept both in the study of personality and in the experimental evaluation of psychotherapy. Regarding the specific methodological problems involved in the use of introspective material, Karl Zener (16) has recently suggested that we can make experiments in using a number of subjects to describe internal or externally referred experiences under conditions which we set up previously and repeat with other subjects. To quote Zener:

> Objectivity requires only that under given specifiable conditions repeatable events, that is, repeatable experiences recur. Whether or not we can directly check a particular experience becomes therefore of only minor importance, if the experience uniformly recurs under adequately specified conditions.

Zener holds that these conditions need not, as in physics, be only specified in terms of external situations. We are here reminded of the vast array of experiments on "set" and of the recent studies on the effect of lysergic acid upon the internal conditions and reported experiences of subjects.

The foregoing remarks, sketchy as they are, should point up the need for a continued careful re-thinking of the appropriateness and scientific merit of introspective methods in the perspective of current psychological theories. This need is being increasingly recognized by psychologists of different persuasions. Each of them has encountered essentially the same problem: his data compel him to take account of relevant experiential contents, i. e., "subjective" and "private" factors in behavior. Prevailing ideology impels him to harmonize this account with the principles of an "objective" and "public" science of behavior. A few representative cases may be mentioned. In a discourse on the definition of behavior, R. H. Waters (15) suggests it be restored to the complete meaning

originally intended by McDougall. If that definition denotes the totality of the behaving organism, then it must include both the overt acts and the covert psychological events. Both are ever present aspects of the total behavior. The observable responses serve as a medium for inferences about inner processes during every day interpersonal contact as well as in scientific analysis. Behaviorists commit an unjustified abstraction by restricting their definition to observable responses. W. W. Meissner (10) comments that psychological constructs often contain implicit meanings that are operationally not made explicit in the construct proper. For instance, the latter may, by implication, represent organismic variables as actual "reality," or conceptualize perceptual responses in a substantive mode. Such "non-constructural aspects of the construct" are, however, not condemned to the cemetery of "surplus meanings" by Meissner. Instead, he tries to salvage them by this corrective: "real" may become operationally legitimate, if it means "real" only for the *Theory*, and experiential aspects of response may become legitimate, if their meanings could be made explicit referents of the construct to be tested. D. Bakan (2) impressively illustrates the whole dilemma in discussing the question: how should the psychologist deal with such a common phenomenon as "Keeping a Secret?" His provocative essay reassesses the reservations against, and the productive use of, introspective material in research. D. C. McClelland (9) reconsiders and tries to solve the problem of a "psychology of mental content." He points to the development of projective techniques as experimental devices to bring the entire dimension of specific contents, i. e., inner states and personal qualities, into "public" psychological focus.

At this point I wish to summarize my main arguments thus far. Experimental methods are not necessarily confined to the strictures of operational definition. This definition itself requires and is undergoing re-examination and revision. Experimental methods can remain objective even with reference to direct experience or reconstruction of direct experience. To this may be added the fact that even where direct experience as reported by the subject is in conflict with actual events (behavior, motivation), this discrepancy is in itself important information for the psychologist — as a fact that he has to take account of and to account for as a scientist.

If one adopts this position, the other question — "how early or late should ultimate experimental validation occur?" — may be answered with a different emphasis on urgency than the dictates of Operationism would indicate. Their influence has often been sterile by tending to nip preliminary exploration in the bud because of the forbidding demand: how do you define operationally your concepts and variables? What are your measures? This tends to restrict the freedom of the psychologist to pursue in his field the problems he sees fit to investigate and to design methods that fit the problem he discovers. It tends to stifle curiosity to inquire into the unique and new that still resist formalized concepts, to inhibit imagination from posing questions that still lack precise definition, and to discourage naturalistic observation. What is easily overlooked in this zest for rigor is the fact that we can investigate only what we have first encountered in the realm of the pre-scientific, that this is the inescapable starting point for any selection, identification, and formulation of the problem for study, and that its explanation and reduction to scientific laws can only follow but not precede the phenomenon we seek to understand. Concerning the alleged priority value of *measurement*, the psychologist might well benefit from this statement by Robert Oppenheimer (11):

> It is not always tactful to try to quantify; it is not always clear that by measuring one has found something very much worth measuring. . . . It is a real property of the real world that you are measuring, but it is not necessarily the best way to advance true understanding of what is going on; and I would make this very strong plea for pluralism with regard to methods . . . (p. 135).

However much one welcomes the operational strategems in advancing scientific objectivity and precision, to make them the sole standards for psychology may prejudge the nature and scope of its subject matter as well as preclude the establishment of methods and concepts appropriate to it. We still have not resolved such basic issues as between nomothetic and ideographic approaches, and between molar and molecular approaches (1, 13). Whether or not, and how, to bring "pre-scientific" phenomena of human existence into the realm of psychology is no less a disputed question. This is exemplified in the issues arising between Neo-positivism and modern Existentialism in its impact on current philosophy, an influence that is reaching us through new, challenging conceptions of psychotherapy.

From such a vantage point it may be more cautious for the psychologist to leave room for phenomenological and other types of descriptive procedures. More often than not, he gets from here the clues and hunches for the formulation of more succinct hypotheses which can be later tested with more rigorous methods. I can only point to the wealth of heuristic material that has come from initial descriptions. There is Rubin's classic demonstration of figure and ground, which we all know to be the starting point of portentous experiments on the human and sub-human level — from Gottschaldt and Katona to Lashley and Krech, etc. There are the pioneering analyses of the world of color and touch perception by David Katz. The laws derived from this type of investigation may be justifiably called "molar" from one point of view, and "low-order laws" from the other. But the historical evidence strongly suggests that we cannot as yet afford to close the door on such approaches. We may not even be in the position to afford closing the door on poorly controlled initial experimentation, if we wish to gain the later fruits of well-controlled experimental verification or repudiation. A typical example is F. Wulf's study of memory traces, with his presumptive findings of levelling and sharpening tendencies. There are few experiments which stimulated so many experimental critiques, pro and con, and Wulf's procedure had many flaws, no doubt. Yet just because of this, his original thesis was productive, and the concepts of levelling and sharpening have proven their value in other research, e.g., by Allport and Postman on rumor, and by Klein, Holzman et al. on cognitives styles of personality.

We all know the immense amount of experimental clinical work today which has been inspired by Freud. One may wonder how Freud would have fared in thinking out his theories if he had written his case descriptions and conclusions under the operational blackout injunctions on speculation in Vienna.

My last point concerns another influence on current research attitudes and thinking of psychologists in the climate of Operationism. This influence concerns the worship of prediction in the use of experiment and the acclaimed control of future behavior. It is not alone the danger of the psychologist becoming one-sidedly absorbed with the power of manipulating living beings (12). Suppose one succeeded in controlling schizophrenia by the Skinner box or curative

chemicals. Should one cease to take stock of the psychological changes in schizophrenia? An equally great danger threatens when the psychologist becomes so fascinated with the lure of hypothesis testing that he may not see the woods of psychological problems for the trees of prediction. The formula: "hypothesis — experimental verification" threatens to become an obsession with professors and their Ph.D. thesis students, so that it looks as if no other tasks exist that merit investigation in psychology, as if no discovery could be made otherwise.

This is not the place to raise further questions about the scientific possibility of explanation without prediction, or of post-diction as in archaeology, in psychopathology, and in biology. There are, however, areas of psychological investigation where systematic observation and data collection, together with non-predictive experimentation, have yielded significant advance in our knowledge. The ecological studies by Barker and Wright, *Midwest and Its Children*, and the ethological work by Tinbergen and Lorenz on animals are outstanding examples. Research in visual perception without prediction in the technical sense has given us the important findings by Adelbert Ames, Ivo Kohler, Pronko and Snyder, and Witkin et al. In developmental psychology the pioneer work by Gesell, by Piaget, and by H. Werner was not carried out with the shibboleth of prediction. The discovery of ontogenetic stages in cognitive and personality development had actually the characteristic that the investigators could not really predict the next stage specifically, but had to explore further.

In view of such accomplishments in different psychological fields, made essentially without prediction, one might ask whether psychologists tend to oversell themselves when they try to emulate only one scientific standard — which upon closer inspection may turn out to be the premature imitation of other, seemingly more mature sciences.

Instead of nursing an addiction to forecasting "which came out, the Lady or the Tiger?" in every case of psychological endeavour, would it not be more modest for each of us to take his cue rather from Voltaire's *Candide* than from Frank Stockton's riddle, and go "cultivate our garden" without telling the other how to do it?

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